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T	1850, July 23·565169	Berlin Mean Time.
	° ′ ″	
$\varpi - N$	180 31 36·74	Mean Eqx. 1850·0.
N	92 53 27·84	
I	68 11 23·56	
e	0·9988519	
q	1·0814488	Motion direct.

leaving the following uncompensated normal differences :—

		$\Delta\alpha \cdot \cos\delta$	$\Delta\delta$	
		″	″	
1.	May 10·0	+5·88	−1·88	
2.	21·0	−3·05	−0·86	
3.	30·5	+0·01	−0·93	
4.	June 7·5	−0·57	+0·67	
5.	21·0	−0·19	+1·67	
6.	July 2·5	−0·07	−0·70	
7.	12·0	+0·47	−0·54	
8.	21·0	−1·06	+1·01	
9.	Sept. 15·0	+1·28	−0·48	The Cape Normals.
10.	Oct. 5·0	−0·36	−0·65	

The period corresponding to the excentricity given above being somewhat short of 29,000 years.

Letter from M. Yvon Villarceau to the Astronomer Royal.

"I have the honour to transmit to you along with this letter a copy of a Memoir which I have just presented to the Academy of Sciences, upon the double star η *Coronæ.* I shall be gratified if you should find it worthy of being presented to the Astronomical Society.*

"Allow me to avail myself of this circumstance to give you some explanations upon a subject of controversy, which appears to have excited the attention of the Astronomical Society.

"In a memoir upon the comet of D'Arrest, I expressed the opinion that the observations of that body, made with the heliometer of Königsberg on the occasion of its last apparition, might be more precise than those which have been obtained elsewhere during the same period. M. Otto Struve appears to conclude from this remark, that, in general, I prefer heliometer observations of comets to those made with a refractor as powerful as that of Cambridge, and he declares that he does not concur with me in adopting this preference. The circumstance which induces him to reject the heliometer in its application to observations of comets consists in

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* An abstract of M. Villarceau's Memoir will be found at p. 229.

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the diminution of the brightness of the images, arising from the duplication of the object-glass.

"I suspect that my excellent friend, M. Otto Struve, has not rightly understood the meaning of my proposition, or, probably, the fault is attributable to myself. In a letter referring to subjects of a rather varied kind, and in which I communicated to him the results to which a discussion of the observations of D'Arrest's comet had conducted me, I did not insist sufficiently upon the point which has become the subject of controversy.

"With the view of fixing the meaning of my proposition, I divide into two classes the modes of procedure employed in observations of comets.

"First, the telescope is fixed in a position such that the comet and the star of comparison ought to present themselves successively in the field of view by the effect of the diurnal motion. With respect to the right ascension, the observation consists in estimating the instants of the passage of the comet and the star across fixed wires, insomuch that the difference of right ascension is based upon the appreciation of instantaneous phenomena.

"Secondly, the telescope is rendered movable in position by means of a clock movement, the star is fixed in the field of view, and the comet ought to be visible in the field at the same time. The observation, according to this method, is made in the same way as when the companion of a double star is observed: or, again, the comet may be considered to be connected by way of triangulation with several small stars visible at the same time in the field. This method, which has been already employed with success, especially by M. Otto Struve, differs from the former inasmuch as the measure of time no longer enters into the determination of the difference of right ascension. The bisection of the image of the comet may be made with the greatest facility by means of an apparatus fitted with wires: the observer may operate at his ease: the instantaneous phenomenon which offers difficulties of observation in the other method does not present itself here: and the time merely serves to fix the date of observation.

"I have discussed, in my Memoir, the different circumstances, according to which the sensibility of the organ of vision plays an important part in the determination of the right ascensions of very faint comets observed by the first method: there are cases in which the right ascensions ought, in general, to fall short of their true values. The optical power of the instrument cannot diminish this inconvenience, except when the comet has ceased to have a certain degree of brightness; but at the moment when it is about to disappear in large instruments, the inconveniences to which I am referring, ought to manifest themselves as well as in instruments of merely moderate power. In the latter, they will manifest themselves several weeks or months earlier, and the comet soon ceases to be observable, while large instruments allow of the observations being continued for a longer time. The errors of right ascension of very feeble comets which have their source in the manner in which the retina is affected during the *passage* of a comet behind a wire, ought, according to my opinion, to disappear entirely if we employ the second method, in which the comet is bisected by a wire, and *remains* in that state during an interval of time, which is enormously longer than that of a passage.

"My opinion, then, is, that better results will be obtained by making use of the second method of observation; that which is, in fact, practised by M. Otto Struve himself. Now, it happens that, in the case of the comet of D'Arrest, the only instrument which has been applied in this way is the heliometer of Königsberg. It is not, however, the heliometer which I extol in so far as regards the comet, but rather the method above referred to. With respect to the heliometer in particular, it may be inferred that if the images are sufficiently bright to be observable without much difficulty, the results may be better than those which would be obtained by employing the first method even with refractors of sufficient power. Now, in the case of the comet of D'Arrest, the heliometer observations will be found to be as concordant as the others; the only difference being, that the observer was compelled to abandon his observations eight days earlier than Mr. Challis at Cambridge.

"If, now, we were to apply the second method, making use of a refracting telescope of the same power as the Northumberland refractor, the observations would unquestionably be more precise than those obtainable by means of refractors of the same dimensions, but fitted with divided object-glasses, since the intensities of the images are in the ratio of 2 to 1. Thus, let it be well understood, it is not the heliometer which I prefer, but the second method of observing comets.

"I regret much that I am unable to lay before you the grounds of the reasoning employed in my Memoir; possibly you might find that I have somewhat exaggerated the inconveniencies of the first method of observation. There is much to be said upon this subject, and, unfortunately, the arguments do not admit of being reduced to figures, which would tend very much to simplify the difficulties.

"Excuse, I pray you, this long digression, which I should not have thought of placing before your eyes, as too uninstructive, if I had not been led to it by the reading of the *Monthly Notices*.

"*Paris, April* 28, 1853."

On the Mechanical Imitation of Precession. By the Rev. Professor Powell, F.R.S., &c.

The actual mechanical exhibition of the composition of rotatory motion giving the physical explanation of the Precession of the Equinoxes has been attempted by more than one contrivance.

In the *Astronomical Notices*, vol. i. p. 43, appears a description of a machine of this kind by Mr. Atkinson, but so brief that it is difficult to collect what the principle of it was. It is also under-